lord-almoner's reader in Arabic for the University of Cambridge. His published writings are principally on philological topics; and the article on 'Shorthand,' in the 'Encyclopaedia Britannica,' is from his pen.

— The twenty-second volume of the 'Encyclopaedia Britannica,' completing the letter 'S,' is now ready. The principal literary and scientific articles are, 'The sonnet,' by Mr. Theodore Watts; 'Sophocles,' by Professor Campbell; 'Spanish literature,' by M. Morel Fatio, the first Spanish scholar in Europe; 'Swedish literature,' by Mr. Gosse; 'Syriac literature,' by Professor Wright; 'Dean Stanley,' by the present dean of Westminster; 'Socrates,' by H. Jackson; 'Stoics,' by D. Hicks; 'Slavs,' by Mr. Morfill; 'Slavery,' by Dr. Ingram; 'Skeleton,' by Prof. St. George Mivart; 'Sponges,' by Dr. Sollas; 'Steam-engine,' by Professor Ewing; 'Sun,' by Mr. Lockyer; 'Surface,' by Professor Cayley; 'Surgery,' by Professor Chiene and three other contributors; 'Spiritualism,' by Mrs. Henry Sidgwick; and 'Sword,' by Prof. F. Pollock.

— The issue of the *Home journal* dated June 15 contains a most complete summer-resort guide. Where to start from, how to go, what it costs to stay, the natural attractions of the different regions, and the accommodations offered by the various hotels at the summer-resorts, are all very clearly and faithfully set down.

— Hon. David A. Wells will contribute to the July *Popular science monthly* the first of an important series of papers on 'The economic disturbances since 1873.' Mr. Wells proposes to review the history of these disturbances, and to point out agencies to which such wide-reaching commercial depression may be properly attributed.

— The two latest monographs issued by the American historical association are 'History of the doctrine of comets,' by ex-President Andrew D. White of Cornell; and 'William Usselinx, founder of the Dutch and Swedish West India companies,' by Dr. J. F. Jameson of Johns Hopkins university.

— The progress made in educating the negroes of the south will be set forth in *The American magazine* for July. The Rev. S. W. Culver, president of Bishop college, Texas, describes the methods of instruction, and the measure of success attained.

— Prof. M. Max Müller's three lectures — 'The simplicity of language,' 'The identity of language and thought,' and 'The simplicity of thought'— given at the Royal institution, London, last March, have been secured for the columns of *The open court*, Chicago. The first of these remarkable lectures was contributed to the May number of the *Fortnightly review:* the other two have not been published, and will be printed for the first time in *The open court*, and from the author's manuscript. The publication of these lectures commenced in *The open court* of June 9.

— The *Harvard university bulletin* announces that the corporation have authorized the publication, through Charles Scribner's Sons, of a memorial edition of the late Prof. E. A. Sophocles' 'Greek lexicon of the Roman and Byzantine periods,' under the oversight of Prof. Joseph Henry Thayer.

— Charles L. Webster & Co., the publishers, sent Mrs. Grant a check for $33,384.53 last week as additional profits on General Grant's 'Memoirs.' She has received thus far nearly $400,000, which is probably the largest amount of money ever earned by the writing of a single book.

— Cupples & Hurd have in preparation a life of Commodore Matthew C. Perry, who was so instrumental in opening the ports of Japan to the world. It will give a complete history of this 'typical naval officer' from the time when, as a midshipman, he served in the war of 1812, to the treaty with Japan.

— Messrs. Macmillan & Co. have published 'Dynamics for beginners,' by Rev. J. G. Lock. This work has been written in the hope of supplying a want, which many teachers have felt, of a book which explains the elementary principles of dynamics, and at the same time illustrates them by numerous easy numerical examples suitable for use in schools with boys of ordinary mathematical attainments. It must be regretted, however, that the author has seen fit to suggest names for the units of velocity and acceleration, as the science of physics threatens to be overburdened with an unnecessary nomenclature.

LETTERS TO THE EDITOR.

**** The attention of scientific men is called to the advantages of the correspondence columns of* SCIENCE *for placing promptly on record brief preliminary notices of their investigations. Twenty copies of the number containing his communication will be furnished free to any correspondent on request.*

The editor will be glad to publish any queries consonant with the character of the journal.

Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

The Charleston earthquake.

The admirable paper on the 'Charleston earthquake' in *Science* of May 20, by Messrs. Dutton and Hayden, is an illustration of what may be accom-

plished by the patient and laborious investigation of a mass of evidence, much of which is unsatisfactory, and not a little of it untrustworthy. That the paper contains so much that is valuable and interesting is greatly to the credit of its authors; and its real importance as the most, indeed the only, elaborate discussion of that interesting seismic event which has thus far appeared, renders a careful examination of its methods and conclusions extremely desirable. I wish to remark upon a few points, concerning which I am compelled to dissent from the views expressed in the paper.

Great labor has evidently been expended on the construction of the isoseismal chart; and doubtless all has been done that can be, with the uncertain data available. Two serious but well-recognized difficulties are met with in attempting the construction of 'intensity' curves: one is the variability and inconsistency of the physical evidences of disturbance, and the other is the unreliability of human testimony as to its extent. In earthquakes of much violence a considerable area near the origin may present evidence which outlasts the disturbance itself, such as overthrown or damaged buildings, chimneys overturned, monuments displaced, etc., and which, therefore, may be studied at leisure. A little experience in the examination of this sort of evidence proves conclusively that a given result is an extremely complex function of a large number of 'independent variables,' most of which, unfortunately, are and must be unknown. It thus becomes difficult to determine the ratio between the varying magnitudes of any one of these variables, so largely is the visible result influenced by the others.

The available source of information consists generally of the effects of the disturbance upon structures of various kinds. Nothing can be more conflicting than the results of such observations, even on areas so small that it seems impossible to admit that differences in actual earth-movement have existed. Within a hundred feet of each other will be found buildings nearly destroyed, and buildings, apparently similar in construction, almost uninjured. Here a monument or shaft is overthrown; and there, a few feet away, another on a much less stable foundation is undisturbed. In a room in which heavy bookcases have been dashed upon the floor, and the furniture generally wrecked, delicate ornaments still rest upon the mantelpiece, and, without crack or scratch, seem to deny all possibility of violent motion. In short, one is forced to the conclusion that the character and amount of destruction caused by an earthquake depend largely on circumstances other than the motion of the earth-particle. An earthquake must be studied in the light of what it has failed to do, as well as of what it has done; and much consideration should be given to what might have happened but did not.

If such widely different effects can be produced by earth-movements which must be practically the same, it is clear that they cannot be very accurate measures of the intensity of seismic disturbances. In a general way, and if extended over an area which includes decided changes in the extent of surface destruction, such observations are extremely useful as indicating zones of unequal disturbance; and especially so, as, in the absence of instrumental records, they furnish about all the available facts.

The nature of the data furnished by the careful and conscientious survey of Mr. Sloan is not stated; but it is perfectly safe to say, that, whatever it may be, Messrs. Dutton and Hayden have made the most of it.

Without intending any special criticism upon the method of treatment adopted, I desire to call attention to the uncertainty, which seems to be great, in the construction of equal intensity-curves with any attempt at precision in form or position, when they are based upon observations of such physical disturbances as are referred to above.

If such records of the disturbance as are left by the earthquake itself are of doubtful and uncertain value, still more so must be the data resting entirely upon the testimony of observers of transient phenomena. It is by no means uncommon for two persons sitting in the same room, and disturbed by the same moderate earthquake, to differ decidedly in their estimate of its intensity.

In two differently constructed or differently situated buildings near to each other the difference is very great. Nor will it do to depend upon the disturbance of movable objects, such as swinging-lamps, etc. Very much depends upon the character of the movement, — as to whether the motion is principally horizontal or vertical, the period long or short, and the synchronism of that period with that of the moving object. Innumerable illustrations of this fact might be given. Disturbances of unusually large amplitude but long period are sometimes scarcely perceptible to the observer. Professor Milne recorded a disturbance in Tokyo on Nov. 23, 1884, of which he says, "Whilst standing up, it was with difficulty perceptible. In the same room, however, those who were seated felt it distinctly. It made a lamp six feet long swing through an arc about six inches."

In 1881 an earthquake occurred at Sapporo (Japan), concerning which the observer made this note: "Wire of hanging-lamp four feet long described an arc of twelve inches; not personally observed; was walking on the street, and nothing was noticed."

Besides the physical environment of the observer, his physiological and psychological peculiarities largely control his estimate of the extent of the disturbance.

In the collection of information by means of distributed circulars, it is impossible to avoid these difficulties, and to obtain any thing like a fair estimate of the character of the phenomenon, especially as most observers are inexperienced. A circular sent to a village is generally likely to find its way into the hands of the particular inhabitant who can give the most startling account of what he saw and felt, and who was naturally most thoroughly frightened.

It appears, therefore, that as far as the value of the collected data is concerned, the great area disturbed by this earthquake might be divided into three zones. The first is small, surrounding and including the epicentrum, and the visible evidences of the intensity of the shock were carefully studied by a sagacious observer within a few weeks of its occurrence. The second consists of the remainder of the area within two or three hundred miles of the epicentral tract, throughout which, though to a constantly diminishing extent, overthrown chimneys, displaced shafts, cracked walls, etc., remained as exponents of the character and magnitude of the disturbance. From this region, however, evidence

came through circular letters, newspaper reports, etc., with which untrained and not very trustworthy observers have much to do. The third zone consists of all that is left of the disturbed area: over it the effects were transient, and all evidence rests on human testimony, unsupported by that of material objects.

Thus it would seem, that, in the construction of the map, isoseismal lines would be drawn with three different degrees of confidence, and that they must be drawn more freely, and with less attention to detail, as they are farther removed from the epicentral tract. Local variations in intensity-estimates should have less weight, and the lines would approximate more nearly to smooth curves. On the map as drawn by Messrs. Dutton and Hayden, this order of things appears to be reversed: the smoothest, most regular curves are those immediately surrounding the epicentrum, while they become more irregular as the distance from that point increases.

In work of this kind, irregular and sinuous lines imply numerous and reliable observations, while those more regularly and uniformly curved will generally be drawn for areas over which observations are few, and not of sufficient weight to show more than the general trend of the line. For these reasons it appears to me that the map is faulty, in that too much weight has been given to individual observations at great distances from the epicentrum; that the sinuosities and irregularities in the lines, particularly those of the Mississippi valley, do not represent any thing real; that they should be smoothed out; and that it is doubtful if sufficient evidence exists for the construction of the two isolated areas, surrounded by closed curves, which appear in northern Illinois and in southern Indiana and Illinois. I venture to suggest, in regard to the latter 'area of silence,' that its existence may to some extent be due to the fact that information concerning that area was collected several months after the occurrence of the earthquake.

One of the most interesting features of this paper is the method employed in determining the depth of the seismic centre. Under certain restrictions, no criticism can be made upon the analysis of the problem; but in its practical application it is, in my judgment, open to serious objection. It is not easy to decide what is the best measure of the 'intensity' of an earthquake. A simple expression for it, and that accepted by Messrs. Dutton and Hayden, is 'the energy per unit area of wave-front.'

This definition once adopted, their analytical and graphic treatment of the problem is elegant and satisfactory; but in the application of the method to the Charleston earthquake, or to any other, it is important to ask whether any means exists for determining the 'intensity' as defined above. While it is true that the disappearance of the consonant *a* from the abscissa of the points of inflection renders it independent of the *absolute* intensity, it must not be forgotten that in determining *relative* intensities the thing to be kept in mind is 'the energy per unit area of wave-front.' As far as can be seen from the contents of the paper, the result depends on the unjustifiable assumption that *surface destruction* is proportional to this. It is a well-established fact that the destructive effects of a motion are not proportional to the energy involved, and in earthquakes many things combine to produce what is ordinarily called the 'intensity' of the shock, or, perhaps better, its 'destructiveness.' An exact expression for this is extremely desirable, and it seems to me that Professor Milne has approximated to it pretty closely in adopting, as he has, the 'maximum acceleration of the earth-particle in a horizontal plane.' There can be little doubt that horizontal movement is more effective in overturning and destroying buildings, chimneys, etc., than vertical; yet the fact, if it be a fact, finds no expression in the method of Messrs. Dutton and Hayden. Their formula and curve demand the maximum intensity at the epicentrum; and this is correct, according to their definition of intensity.

But does the greatest destruction take place at the epicentrum, or is it to be found in a zone whose radius depends on the depth of the seismic centre? I would not venture to place my own judgment, based upon a hasty examination along a single line, against that of an observer who has gone more leisurely over the field; but, as I can nowhere discover in the paper a distinct statement as to where the *most destructive effects* were observed, I may remark that it appeared to me that there was much less destruction in the neighborhood of the epicentrum, where the vertical component of the motion seemed to have predominated, than in and about the city of Charleston.

Of course, it is possible that from a study of the surface disturbance the relative amount of energy per unit area of wave-front at different points may have been worked out, and the point of inflection found from these results; but it would be an extremely complex problem, and, in addition to difficulties already suggested, it is complicated by the fact that the normal motion of the particle must be changed as the wave emerges from the earth: this, indeed, would stand in the way of getting just what is desired from perfect instrumental records, as, at best, they can only reveal surface movements.

I am unable to agree with the conclusion of Messrs. Dutton and Hayden expressed in the statement that the amplitude of vibration of the earth-particle was in some places not less than ten inches or a foot. So large an amplitude appears to me to be extremely improbable. It is only within a few years that any thing like accurate measures of amplitude have been made; and it is well known, that, wherever it has been measured, it has been found to be small.

In the 'general run' of Japanese earthquakes, the amplitude has been found to be not much greater than a millimetre, and often less. In a few cases it has been several millimetres; and I believe in one or two, which were nearly 'destructive,' and by which chimneys were overthrown and walls cracked, it has been as high as ten or twelve millimetres. It will be noticed, however, in examining these reports, that, in most of the cases in which large amplitudes are reported, the disturbances were of unusual length.

Although, in the construction of their numerous 'steady-point' seismographs, Messrs. Ewing (I want to be careful not to put any one of these names first) Gray Milne have well-nigh revolutionized the science of seismology, I am inclined to the opinion that in a prolonged disturbance the 'steady point' is likely to be set in motion, and that a magnification of the amplitude may sometimes result. A very large

amplitude is to my mind incompatible with innumerable observations of *what did not happen* in Charleston. I admit the difficulty of the problem, but think it easier to account for large displacements by successive movements of small amplitudes.

I must also dissent from the opinion expressed as to the value of stopped clocks as a means of determining the time of the wave-transit. Is it not likely that most of the inconsistencies which appear on a comparison of such data arose out of the fact that many of the clocks were not correctly regulated to 75th meridian time, or that their errors were not known? The man whose clock or watch is 'just right' is met with at every turn, especially after an earthquake; but to most people this means that the error is not greater than a minute or two.

If all of the stopped clocks in the area disturbed had been in exact agreement before the shock, I do not think the errors would have been very great; except, perhaps, in the immediate vicinity of the source. The stopping of all clocks at any considerable distance probably occurred at the transit of the same great wave. Of course, a properly adjusted seismoscope with a clock attached is infinitely better, but I do not have great confidence in the 'observer with watch in hand.' Most intelligent observers in this country must be classed as inexperienced: the watch is not generally in his hand until after he is convinced that the something which has happened is an earthquake, and then it is very likely to have a large and unknown error. Should the disturbance be so considerable as to threaten to be destructive, the skill of the observer in 'measuring a part of the shock and estimating the beginning' is tolerably certain to be overshadowed by his disposition to seek a place of safety. The position and environment of the observer at the time of the occurrence will greatly influence the character of the phenomenon. As an illustration, I may compare my own observations with those of Professor Newcomb, when the Charleston earthquake was felt in Washington City.

I was seated in my library on the second floor of a three-story brick building, about four squares from the state, war, and navy building. As soon as the disturbance was felt, the time was noted. In a moment the motion became very strong. My small boy, who had been awakened out of a sound sleep, rushed into the room; and the family quickly decided to do what it had often done before under such circumstances, and found its way to the street. By the time this was accomplished all was quiet; and in two minutes from the beginning we were again seated in the same room, discussing the shock. In a few minutes, about five from the beginning, another shock occurred, much less violent than the first.

Professor Newcomb "observed a duration of perceptible tremors, with two maxima lasting about five and one-half minutes."

There is, of course, no doubt but what these tremors were felt, but it may be a question whether they were prolonged vibrations of the building in which Professor Newcomb was, or real earth-movements. I am pretty sensitive to earthquakes, and I can say with certainty that they were not felt by me or by my family.

Everybody, I am sure, will agree that it is highly important to establish a large number of observing-stations, equipped with the best instrumental appliances which can be obtained. Even so small a number as ten or twenty such stations, well distributed over the area disturbed by the Charleston earthquake, would have put us a long way in advance of our present knowledge of seismology. It is greatly to be hoped that the able and interesting discussion of the subject, which Messrs. Dutton and Hayden have evolved from the mass of observations which they have gathered with so much industry, will serve to direct the attention of intelligent people to the importance of such a system of observing-stations, and that in the near future the director of the geological survey will be enabled to establish it.

T. C. M.

Terre Haute, June 1.

Museums of ethnology and their classification.

The remarks of Dr. Boas and Professor Mason on the classification of ethnological material raise questions which must occur to every one who has before him unclassified material. As both views include a part of the truth, the decision on the course to be adopted must depend upon the amount of material to be handled, the space available for its exhibition, and the purpose most at heart in the organization of the museum considered as an agency for effecting a purpose.

The ideal way, if all circumstances were favorable, would be to have a double series,— one representing the culture of each people as an ethnic unit; and the other a comparative collection illustrating the relations to a common standard of the items making up each tribal aggregation. In ninety-nine museums out of a hundred, this would be impracticable, owing to the expense involved, the exhibition space required, and the difficulty of obtaining sufficient duplicate material for two series. The decision must therefore depend on the object to be attained. Is this to show the manner in which tools, weapons, dress, etc., have been elaborated, under the operation of the environment, by the human mind in varying stages of development, or is it rather to convey to the observer the resultant of all the forces acting in and on a comparable series of ethnic types or units, each complete in itself? In either case the object is a worthy one, and to be attained in its particular manner. Neither is likely to be completely attained under the existing conditions of museums in this or any other country; but, as attempted in different collections, we may regard them as complementing each other. In the one case, as very truly observed by Dr. Boas, we are helped to a knowledge of what problems exist; and it is no little matter to have a rational sailing-direction over a trackless ocean, though the accurate chart is still to be made. In the other, we have the equivalent of the monographic study of the specialist who surveys in detail, and for all time, a gulf or harbor forming a small part of the oceanic coast.

To conclude, for the people at large and the majority of those who profit by public museums, I believe the greatest amount of satisfaction and instruction is to be obtained rather from an ethnic arrangement than from the organic method; but this is merely an expression of my individual preference.

WM. H. DALL.

Washington, D.C., June 4.

Prof. Otis T. Mason's reply to my remarks on his views of the methods of ethnology is mainly a justi-